REFERENTIAL DESCRIPTIONS AND CONVERSATIONAL IMPLICATURES

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ABSTRACT

The standard Gricean defense of Russell in the face of referential uses of descriptions has been to claim that these uses are conversational implicatures. My argument in "The Case for Referential Descriptions" focused on showing that these uses are not particularized implicatures. I now adapt and develop this argument to show that they are not generalized implicatures nor otherwise pragmatic. I consider and reject Bach's contrary proposal in "Descriptions: Points of Reference." Finally I look critically at Neale's new view in "This, That, and the Other" that the debate between referentialists and Russellians is "the product of a powerful illusion."

Key words: Donnellan, definite descriptions, indefinite descriptions, referential uses, attributive uses, implicatures, demonstratives, Bach, Grice, Neale, Russell

1. Introduction

Under the influence particularly of Keith Donnellan (1966, 1968), many think that definite descriptions, "definites", are "ambiguous", having not only the "attributive" quantificational meaning captured by Russell[1] but also a "referential" meaning like that of a name or demonstrative. Under the influence particularly of Charles Chastain (1975),[2] some now think the same of indefinite descriptions, "indefinites".

It is generally agreed that descriptions have a referential *use* as well as an attributive *use*. When 'the *F*' is used attributively in 'The *F* is *G*' the sentence conveys a thought about whatever is alone in being *F*; when 'an *F*' is used attributively in 'An *F* is *G*' the sentence conveys a thought about some *F* or other. The sentences convey "general" thoughts or propositions. When either description is used referentially, its sentence

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[1] Any differences that there may be between what Donnellan says of 'attributive' descriptions and Russell's theory of descriptions are beside the point of this paper.

[2] See also Strawson 1950, Wilson 1978.

conveys a thought about a particular *F* that the speaker has in mind, about a certain *F*. The sentences convey "singular" thoughts or propositions.[3]

Despite agreement that descriptions have these two *uses*, there is no agreement that they have two *meanings*. The quantificational attributive meaning described by Russell is uncontroversial, but many, particularly Stephen Neale in *Descriptions* (1990),[4] have appealed to ideas prominent in the work of Paul Grice (1989) to deny that descriptions also have a referential meaning. They argue that the referential use of a description does not affect "what is said" by its sentence. For what is said is the content of the Russellian general thought. The content of the singular thought is indeed conveyed but only as a "conversational implicature". So what is thereby conveyed is not the meaning of the sentence on this occasion and hence not the concern of semantics; rather it is the concern of pragmatics.

In "The Case for Referential Descriptions" (2004) I claim that the case for the thesis that descriptions have referential meanings has been greatly underestimated. I argue that the referential uses of both definites and indefinites constitute referential meanings: the uses are semantically significant, not merely pragmatically so. I call this thesis 'RD'. It is a consequence of RD that descriptions are, in some sense, "ambiguous". A key part of my argument for RD is the rejection of the above Gricean defense of Russell ("Argument I" in sec. 2).[5] In "This, That, and the Other" (2004) Neale calls this sort of rejection "the Argument from Convention" and concedes "an important point" to it (p. 71); "I no longer think the difference between *saying* and *meaning* lies at the heart of a characterization of referential usage." This does not, however, lead him to embrace RD. Indeed, he still thinks that "the Russellian analysis is basically correct for both attributive and referential uses of descriptions" (p. 106). Furthermore he now thinks that debate over RD "seems to lack real substance" (p. 71); it "is the product of a powerful illusion" (p. 106).

I shall look critically at Neale's new position in the concluding section 8 of the paper. But most of this paper will be concerned with two other matters. First, the focus of my presentation of the Argument from Convention was on the idea that referential uses are *particularized* conversational implicatures. I now think that this focus was a mistake. In section 2 I shall summarize that presentation. In sections 3 to 5 I shall adapt and develop the argument so that its focus is on the idea that referential uses are *generalized* conversational implicatures. In sections 6 and 7 I shall respond to Kent Bach's "Descriptions: Points of Reference" (2004). Bach defends the Russellian *status quo* against the Donnellan-inspired revolution in general, and against my version of the revolution in

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[3] Some philosophers call these thoughts and propositions 'de re', others, 'object-dependent'. I think that there are reasons against both usages (1996, p. 144n; 1985, sec. 3). And in calling them "singular", I don't mean to commit to the view that their content includes the object in mind rather than the causal mode of presenting that object (1996, ch. 4).

[4] For some others, see Kripke 1979, King 1988, and Ludlow and Neale 1991.

[5] The following are earlier presentations of such a rejection: Devitt 1997a, pp. 125-8; 1997b, p. 388; Reimer 1998.

particular. It seems to me that Bach's defense of the *status quo* strengthens rather than weakens the case for the revolution.[6]

The discussion will all be about definites but much of it can easily be applied to indefinites.[7]

2. The argument from convention

Donnellan and others produced a number of examples of the referential use of definites that led many to embrace RD. The Gricean response to these examples made this embrace seem too hasty because it raised the possibility that all these referential uses could be explained pragmatically. Neale has argued persuasively that a pragmatic explanation is the *right* one by making a comparison with other quantifiers (1990, pp. 87-91). Thus, consider 'every':

> Suppose it is common knowledge that Smith is the only person taking Jones' seminar. One evening, Jones throws a party and Smith is the only person who turns up. A despondent Jones, when asked the next morning whether his party was well attended, says,
>   (7) Well, everyone taking my seminar turned up
> fully intending to inform me that only Smith attended. The possibility of such a scenario, would not lead us to complicate the semantics of 'every' with an ambiguity; i.e., it would not lead us to posit semantically distinct quantificational and referential interpretations of 'everyone taking my seminar'. (Neale 1990, pp. 87-88)

Similarly, the possibility of Donnellan's scenarios should not lead us to complicate the semantics of 'the *F*'. Neale goes on to argue that Grice's pragmatic theory of conversational implicature explains the mechanism by which, in all these scenarios, the speaker conveys a meaning that his words do not literally have. Thus, the theory explains how Neale, by assuming that Jones is acting in accordance with "the Cooperative Principle"

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[6] All unidentified page references to the works of Neale, Bach, or me are to our 2004 papers.

[7] And I do have a comment on Bach's discussion of indefinites that is additional to my earlier comments (pp. 293-294) on his point 6 (pp. 205-206). Almost all uses of indefinites that I call "referential" Bach calls "specific" with the result that referential uses are not regular as my argument requires but "relatively rare". This is more than a verbal difference. He calls these uses specific not referential because "the listener cannot engage in 'reference-borrowing' in order himself to refer to the individual in question, for there is no reference to borrow" (p. 225n). He does not say why there is no reference to borrow given that, as all agree, the speaker has a certain object in mind in using the indefinite. In any case, I claim that reference can be borrowed. A good reason for thinking this is that reference is borrowed in analogous situations with proper names and natural kind terms (and, I might add, definites): as I pointed out, "someone with no independent capacities to refer to Catiline or elms can borrow these capacities from a speaker's use of 'Catiline' or 'elm'" (p. 293). Perhaps Bach denies this, but it is very well supported, particularly by the arguments from ignorance and error. If he does not deny it, it is hard to see what basis he could have for thinking that the reference of an indefinite cannot be borrowed.

and its maxims, derives the implicature (speaker meaning), **Only Smith turned up**, from what Jones literally said (conventional meaning). (Grice's Cooperative Principle is: "Make your conversational contribution such as is required, at the stage at which it occurs, by the accepted purpose or direction of the talk exchange in which you are engaged"; Grice 1989, p. 26.).

The Argument from Convention is a response to this nice point. I presented the core of the argument as follows:

> The basis for RD is not simply that we *can* use a definite referentially, it is that we *regularly* do so. When a person has a singular thought, a thought with a particular *F* object in mind, there is a regularity of her using 'the *F*' to express that thought. And there need be no special stage setting enabling her to conversationally imply what she has not literally said, nor any sign that her audience needs to use a Gricean derivation to understand what she means. This regularity is strong evidence that there is a *convention* of using 'the *F*' to express a thought about a particular *F*, that this is a *standard* use. This convention is semantic, as semantic as the one for an attributive use. In each case, there is a convention of using 'the *F*' to express a thought with a certain sort of meaning/content. (Devitt p. 283)

So the idea is that 'the' is ambiguous, having both a quantificational meaning that yields attributive definites and a referential meaning that yields referential definites.

Just how regular and standard are referential uses? I think, as does Bach (p. 220), that most uses of definites are of "incomplete" ones (to be discussed in section 5). I think also that almost all those uses are referential. All in all, setting aside superlatives and anaphoric uses, I'd guess that the vast majority of uses of definites are referential.

This argument still seems to me sound and to make RD very plausible. Yet, by focusing on regularity and special stage setting, it leaves itself open to a possible objection along the following lines:[8]

> The argument may show that referential uses are not examples of *particularized* conversational implicatures like Neale's example (7) but it does not show that they are not examples of *generalized* conversational implicatures. For, particularized implicatures are *not* regularities and *do* require special stage setting, as Neale's example shows. But generalized implicatures are different in both respects. As Grice says, in the generalized case "the use of a certain form of words in an utterance would *normally (in the absence of special circumstances)* carry such-and-such type of implicature" (1989, p. 37; emphasis added); and the generalized

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[8] For example, see Bach, pp. 227-228.

case is, as Neale says, "relatively independent of the details of the particular conversational context" (1990, p. 83).

"Case" did not consider an objection in this form. However, it did provide some of the tools for dealing with it.

"Case" pointed out (pp. 285-286) that to defend the Russellian view of referential uses it is not enough to show that these uses *could* be explained pragmatically as generalized conversational implicatures. We need an argument to show that it *should in fact* be explained in this way. I knew of no such argument. Thus, although Neale seems to suggest that referential uses are generalized implicatures (1990, pp. 81, 90), his argument is concerned with particularized implicatures. Bach has now produced an argument that explains referential uses as "akin to" generalized implicatures (p. 227). I will consider this argument in section 6. But first, over the next three sections I shall present an argument against all such pragmatic explanations. Part of my argument will be, of course, to emphasize the strengths of the explanation presented by RD.

3. Distinguishing implicating from saying

We need to be clearer about the issue, in particular about the crucial Gricean contrast between *what is said* and *what is implicated*. About the former, Grice says that "what someone has said" is "closely related to the conventional meaning of the words (the sentence) he has uttered" (1989, p. 25). Allowing for the fact that a sentence may have more than one conventional meaning and hence be ambiguous, we can capture this close relationship by noting that what is said by an utterance of a sentence, *S*, is determined by the conventional meaning of *S* according to the semantic conventions participated in by the speaker in making the utterance, together, of course, with whatever fixes indexical, demonstrative, and anaphoric reference.[9] Now suppose that *S* contains a definite, and abstract from any ambiguities in the rest of *S*. We have to choose between two hypotheses about an utterance of *S* where the definite is used referentially. (i) The Gricean pragmatic hypothesis is that *S* is not ambiguous. In this utterance the speaker says that *p*, a general proposition, and conversationally implicates that *q*, a singular proposition. (ii) The RD semantic hypothesis is that *S* is ambiguous. In this utterance

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[9] Pragmatists like Sperber & Wilson (1995), Carston (2000), and Recanati (2004) think that more factors go into determining what is said, having in mind particularly the ways in which utterances are elliptical. I am sympathetic to this line of thought in general. However, I don't think it applies to referential uses of definites in particular. My criticism of the explicit and implicit Russellian approaches to incomplete referential definites (pp. 297-303), summarized in section 5 below, amounts to an argument against one such pragmatist proposal. Recanati (1989) makes another proposal which strikes me as very implausible for the reasons set out by Neale (1990, pp. 110-112n). Bezuidenhaut (1997) is a similar proposal. Neale's latest view that referential uses demand "Gödelian completions", discussed in section 8 below, is another pragmatist attempt to save Russell. The Argument from Convention is that referential uses, like demonstratives, contribute to what is said because of convention and reference fixing and hence not as the result of any pragmatic enriching.

the speaker says that *q* and does not say that *p*, but had he participated in the convention for attributive definites he would have said that *p*. How can we choose between (i) and (ii)? Indeed, more generally, how can we distinguish cases of implicating from cases of saying?

Perhaps Grice's characterization of conversational implicature will help:

> A man who, by (in, when) saying (or making as if to say) that *p* has implicated that *q*, may be said to have conversationally implicated that *q*, provided that (1) he is to be presumed to be observing the conversational maxims, or at least the Cooperative Principle; (2) the supposition that he is aware that, or thinks that, *q* is required in order to make his saying or making as if to say *p* (or doing so in *those* terms) consistent with this presumption; and (3) the speaker thinks (and would expect the hearer to think that the speaker thinks) that it is within the competence of the hearer to work out, or grasp intuitively, that the supposition mentioned in (2) is required. (Grice 1989, p. 31)

Neale finds two further conditions in Grice: (4) implicatures "are *intended*", which is "intimately connected to condition (3)", (1992, p. 528); (5) implicatures are "cancelable". Neale thinks that these five conditions come "as close as we can with Grice's machinery to a set of necessary and sufficient conditions on conversational implicature" (1992, p. 529).

Grice's characterization tells us what it is for a speaker to conversationally implicate that *q given that he is saying that p* and so is clearly not *designed* to distinguish saying and implicating. The characterization takes for granted part of what needs explaining: what it is to say that *p*.[10] So we probably should not expect it to be much help in choosing between our hypotheses. Still, it is illuminating to see how difficult it is to get it to help.

Set aside (3) for a moment. (1) and (2) are no help because they also play a role in handling ambiguities of the sort posited by hypothesis (ii): a speaker thinks that he must say that *q* and not *p* by an ambiguous *S* if he is to observe the Cooperative Principle (or something very similar). (4) and (5) are no help either: on hypothesis (ii) the speaker clearly intends *q* by *S* since that is what he said; and that meaning could have been cancelled by saying something further should the speaker have wanted to say *p*.

Is condition (3) a help? Intuitively there has to be something different in the state of mind of a speaker who, in participating in a convention for *S* in order to say that *p*, *implicates* that *q* from that of a speaker who participates in another convention for *S* in order to *say* that *q*. And there has to be something different in the state of mind of a hearer who grasps that *q* when it is implicated from that of a hearer who grasps it when it is said. (3) seems relevant to this because it is concerned with the states of minds of the speaker and hearer

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[10] What about Grice's parenthetical "makings as if to say"? I shall set this complication aside until later (sec. 5, note 15).

in a conversational implicature. So, let us examine (3). Unfortunately, it is not as clear as it might be.

Consider Neale's (7) in section 2. What makes it such a convincing example of an implicature is that it is plausible to suppose that Jones thinks that the hearer, Neale, will go through a Gricean derivation to grasp what is implicated - *Only Smith turned up* - and plausible to suppose that Neale actually does go through that derivation in grasping that implicature; it is plausible to think that the thought is "psychologically real" in the speaker and the derivation in the hearer. So condition (3) is *very clearly* fulfilled. And this is typically the case with particularized conversational implicatures. But it is not with generalized ones; for example, those involving 'and' and 'some'. Consider two cases. First, suppose that instead of (7) Jones says "Smith turned up and got drunk", implying that he *first* turned up and *then* got drunk. Second, suppose that Jones has lots of people in his class and responds to Neale's question by saying "Some people in my class turned up", implying that some in his class did not turn up. Now it seems rather unlikely that Jones thinks, even in the dispositional sense that does not require an actual thought process, that Neale will grasp what is implied in each case *by going through the relevant Gricean derivation*; and it seems rather unlikely that Neale actually does go through it. Yet these cases are supposed to be paradigms of generalized conversational implicatures. How is condition (3) fulfilled in these cases? And is it fulfilled in a way that preserves the distinction between implicating and saying?

(3) does not, of course, require the speaker to think that the hearer will actually go through the derivation but merely that the hearer is *competent* to do so, or at least *competent to "grasp intuitively"* that the speaker realizes that, in order to comply with the Cooperative Principle in his utterance, he must intend to convey that *q*. Let us look further into this, starting with the former competence-to-derive alternative.

In "Case" (pp. 284-5) I considered two matters that are fatal to this alternative and relevant to assessing (3) in general. The first is a *reductio* built around a character I call "a fundamentalist Gricean". The fundamentalist uses a Gricean argument about the beginnings of language to claim that *there are no conventional meanings at all: it is pragmatics all the way down*.[11] Let us set this *reductio* aside and focus on the second matter, dead metaphors. A metaphor is a Gricean paradigm: a derivation from the conventional meaning yields an implicature that is the metaphorical speaker meaning. In time, a metaphor often "dies": an expression comes to mean conventionally what it once meant metaphorically. So, there is then an ambiguity. This is not a rare phenomenon. Indeed, a large part of language is made up of dead metaphors. Yet, of course, in each case there is still a Gricean

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[11] Bach claims that this "reductio is based on a bad analogy. In a given communication situation the hearer's job is to figure out how the speaker is using a certain expression, not how it could have come to be used that way" (p. 178n). This is beside the point of the reductio. That point was as follows: showing that we, or even speakers, can provide a Gricean derivation of the meaning conveyed by a particular use of an expression does not establish that that meaning is not a conventional meaning of the expression; it does not establish that this use of the expression is a matter of pragmatics not semantics.

derivation of the new meaning from the old. Furthermore, and this indicates the problem, *many people could work out the derivation*. Marga Reimer has a nice example (1998, pp. 97-98). The verb 'incense' still means *make fragrant with incense* although it is now more commonly used to mean *make very angry.* That use was once metaphorical, explained by a Gricean derivation. And many could still explain it that way, as Reimer demonstrates. Many probably couldn't, of course, but that is irrelevant because their ignorance could be removed by a bit of education. So, with education, we could all become competent to give the derivation and we could all think that we were all competent. Yet, despite this, a speaker using a sentence containing that dead metaphor to convey the message that *q* could still be *saying not implicating* that *q*. A well-known general competence to derive a meaning that was once metaphorical does not prevent that meaning from now being conventional.[12]

The moral to draw from this is that if conversationally implicating that *q* is to be distinguished from saying that *q* we need a more psychologically demanding condition than (3)'s competent-to-derive alternative. Merely thinking that a hearer is competent to go through a Gricean derivation is not enough for implicating rather than saying that *q*: merely being competent to go through the derivation is not enough for a hearer grasping *q* as implicated rather than said. The derivation must have a more substantial role in a speaker and hearer. So we still haven't provided the help we need to choose between the Gricean hypothesis (i) and the RD hypothesis (ii).

The Gricean conditions (1) to (5) might seem to be necessary and sufficient for a speaker to conversationally implicate that *q given that he is saying that p*, but attention to dead metaphors clearly shows that the conditions are not, as they stand, sufficient to distinguish conversationally implicating that *q* from saying that *q*. And that is something that we should demand from a satisfactory characterization of conversational implicature. It is not acceptable for a characterization to simply take what is said for granted.

We could doubtless get the distinction we want by strengthening (3) to require that a speaker who conversationally implicates that *q* thinks that the hearer *will* go through the Gricean derivation of *q*. But the trouble with this stronger requirement, as noted, is that it seems unlikely to be met by paradigm generalized conversational implicatures – for example, those involving 'and' and 'some'. To save these implicatures we need a weaker requirement but nonetheless one that is more demanding than the competence-to-derive alternative. Can we get help from condition (3)'s competent-to-grasp-intuitively alternative?

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[12] Bach takes my discussion of dead metaphors in "Case" to be drawing an analogy between them and referential uses of descriptions and hence to be making a point aimed at the Russellian view in general (p. 226). But that was not the point of my discussion. The point was aimed at a particular Gricean *argument* for the Russellian view, the argument that we shouldn't treat these uses as a semantic convention because they can be explained as implicatures. And, we might add, the point counts fairly decisively against an earlier Bach claim (1995, p. 683) that, whereas in pragmatic cases speakers have the information to go through a Gricean derivation of the meanings conveyed, they do not in semantic cases.

This alternative raises a question. When the speaker thinks that the hearer is competent to grasp the derivation intuitively, and where the hearer is competent to do so, what is the significance of the Gricean derivation? Grice addresses this question:

> The presence of a conversational implicature must be capable of being worked out; for even if it can in fact be intuitively grasped, unless the intuition is replaceable by an argument, the implicature (if present at all) will not count as a conversational implicature; (Grice 1989, p. 31)

"Capable of being worked out" *by whom*? It is obviously not enough that *we theorists* could provide the derivation, which amounts to requiring just that there *be* a derivation. (If one has any doubts about this, dead metaphors should remove them: dead metaphors have derivations and yet are conventional.) We need *the speaker and hearer* to be somehow involved with the derivation. What might that involvement be?

Let us start by considering how a hearer might be involved with a derivation when she grasps it intuitively. In noting that hearers probably do not go through the derivation in the cases thought to be paradigm generalized conversational implicatures we are implicitly taking the missing processes to be conscious rational ones "in the central processor". But the speedy automatic part of language processing, which is most of it, is subconscious and peripheral; it is more brute-causal than rational-causal. This applies not only to syntactic and semantic processes arising simply out of "linguistic knowledge" but also to pragmatic processes arising out of "world knowledge", including those processes that disambiguate and assign referents to indexical and demonstrative expressions.[13] So perhaps grasping a Gricean derivation intuitively is like that. "Without thinking", a hearer goes through what may be a fairly brute-causal process that takes appropriate account of the clues for a Gricean derivation, perhaps without even representing these *as* clues: she has become "hard-wired" to process these clues appropriately. This speedy subconscious process "mirrors" the rational process of a Gricean derivation. Finally, the suggestion is that this subconscious process of grasping an implicature is very different from the process of understanding what is said.

Given this account of grasping a derivation intuitively how can we ease our requirement on conversationally implicating that *q* so that it might be met by alleged generalized implicatures? We must not insist on the speaker thinking that the hearer will consciously go through the derivation. Rather, the suggestion should be that it is sufficient if the speaker thinks that the hearer will grasp the Gricean derivation intuitively, in the way just described. Is it sufficient that the speaker thinks that the hearer is merely *competent* to grasp the derivation intuitively, as condition (3) allows? I can see no reason to think so and dead metaphors give us a reason to think not. So the competence-to-grasp-intuitively alternative, as it stands, like the competence-to-derive alternative, should be abandoned. My

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[13] For a discussion of the current state of research into language use, see Devitt 2006, ch. 11.

final suggestion is that, although the speaker's attitude to the hearer may be a conscious thought, it need not be. It is sufficient that the speaker's speedy automatic subconscious production of language simply treats the message that *q* as if the hearer will go through the derivation or grasp it intuitively. It is sufficient, we might say, that the speaker "grasps intuitively" that the hearer will do this.

In sum, with "grasp intuitively" understood in the way described, the suggestion is that we modify condition (3) as follows: the speaker thinks or grasps intuitively that the hearer will work out or grasp intuitively, that the supposition mentioned in (2) is required. If we adopt this suggestion it is plausible to think that the process of conversationally implicating that *q* is different from the process of saying that *q*.

So we seem to have a contrast between implicating and saying that *could* save alleged generalized conversational implicatures. And it is perhaps plausible that, for example, our use of 'some' to convey *some not* is to be explained by our "automatically" following the Maxim of Quantity ("Make your contributions as informative as is required"; Grice 1989, p. 26). But, unfortunately, we know too little about language processing to be confident that the contrast *does* save all these alleged implicatures. We simply do not know what is going on at the subconscious level in producing sentences involving 'and', 'some', and the like. Related to this, at first sight the contrast seems to be no help at all with our choice between the Gricean hypothesis (i) and the RD hypothesis (ii). Hypothesis (ii), which takes a referential use of a definite to be like that of a complex demonstrative (sec. 7 below), predicts that its processing will be of the type to be found in saying that *q* whereas hypothesis (i) predicts that it will be of type found in conversationally implicating that *q*. We seem to know too little about language processing to tell which of these predictions is correct.

However, there is one important feature of our distinction between saying and implicating that is a help. The feature is that in conversationally implicating that *q* the speaker *also says something else*, whereas in simply saying that *q* he need not. So if we can show that a person who somehow conveys that *q*, does *not* also say something else then we have shown that the person is not implicating that *q*. This does not settle the fate of the paradigm generalized conversational implicatures but it does help the choice between hypotheses (i) and (ii). For, if we can show that a person who utters *S* with a definite used referentially, thus conveying somehow the singular proposition that *q*, does *not* also say a general proposition that *p*, then we have shown that (ii) should be preferred to (i). Indeed, we can generalize. The feature noted will be found in *any* pragmatic explanation of a speaker's conveying the proposition that *q* without saying that *q* (ignoring messages that are conveyed without saying anything), whether the explanation relies on the notion of conversational implicature or on some other pragmatic notion. Let us simply call all such explanations, "pragmatic". The generalization then is: if we can show that a person who utters *S*, conveying somehow the singular proposition that *q*, does *not* also say a general proposition that *p*, then we have shown that (ii) should be preferred to *any* pragmatic explanation. (i) is simply an example of such an explanation.

I think the Argument from Convention makes it plausible that there is a referential convention and hence that in uttering *S* the person says that *q* and *not* that *p*; it makes RD plausible. In section 5 I shall add to this argument against the view that the person would be saying that *p*. In sections 6 and 7, I shall consider Bach's Russellian view and criticism of RD. But first, in light of this discussion of implicating and saying, I want to take up matters of conservatism and onus.

4. Conservatism and onus

I suggested in "Case" that discussions of referential uses reflect "a conservative climate of opinion that takes the task to be simply to defend the Russellian status quo from criticisms rather than to examine the independent merits of the Donnellan-inspired alternatives" (p. 289). This conservatism accepts no onus to show that the Russellian view is *better* than its referentialist rival. The conservatism is clearly supported by Grice's Modified Occam's Razor. And consider the following passage:

> Though it may not be impossible for what starts life, so to speak, as a conversational implicature to become conventionalized, to suppose this is so in a given case would require special justification. (Grice 1989, p. 39)

We note first that, given the plenitude of dead metaphors, the phrase "may not be impossible" is far too grudging. More importantly, there is no basis for thus putting the onus on showing that conventionalization *has* taken place rather than on showing that it *has not*, so we still have an implicature. Given dead metaphors, and the absurdity of fundamentalism, it seems appropriate that, in general, the onus should be equal. And in the particular case of referential definites, I think that the Argument from Convention places the onus squarely on the Russellian: the Argument makes it *prima facie* plausible that there is a convention for referential uses.

Bach's initial characterization of our disagreement is revealing on this score. He takes the disagreement to be over the view that "special conventions or referential meanings are not needed to enable descriptions to be used referentially". He is convinced of this view, but I am alleged not to be (p. 223). But this is not my position. I do not think that we *need* referential meanings to use descriptions referentially. I accept, for good Gricean reasons, that any quantifier can be used referentially; see Neale's (7) for an illustration. So, I accept that descriptions, as construed by Russell, can be used referentially (p. 283). More generally, although "Case" does offer criticisms of the Russellian treatment of referential uses, my main case for RD is not that the Russellian cannot explain the phenomena. My main case is positive (pp. 281-290) and might be summed up in these words: *RD provides the best explanation of the phenomena of referential use*. More is required to rebut this than fending off criticisms of Russellian explanations. The Rus-

sellian has an onus to show that his explanation is *better*. I am attempting to show that the Russellian explanation is in fact much worse.

Despite this mischaracterization of our disagreement, Bach accepts the onus, as we shall see in section 6.

5. *Further arguments against pragmatic hypotheses*

Main argument. If a pragmatic hypothesis like (i) were true then a person using a definite referentially in uttering *S* would convey the singular proposition that *q* while saying the general proposition that *p*. So it is sufficient to refute such hypotheses to show that the person does not say that *p*. I think that a consideration of incomplete definites shows this very persuasively.

An incomplete definite is one that fails to uniquely describe an object; for example, 'the table' in 'The table is covered with books'. Incomplete definites pose a problem for Russell's theory of descriptions because, typically, they seem to pick out an object despite this failure. Two sorts of modification to Russell have been proposed to cope with the problem, what Neale calls "the explicit approach" and "the implicit approach". According to the explicit approach, an incomplete definite is elliptical for a longer description that the speaker could supply. According to the implicit approach, "the context of utterance delimits the domain of quantification" of the definite (1990, p. 95). It is not in contention that one or other approach must be right for attributively used incomplete definites. However, I argued in "Case" (pp. 297-303), neither approach works for referentially used ones.

The same sort of "ignorance and error" arguments that are devastating for description theories of proper names are also devastating for any Russellian theory of referentially used incomplete definites. Yet, interestingly, these arguments have been largely ignored by defenders of Russell. Here is the recipe for generating the arguments. Take a referential use of an incomplete 'the *F*' where the object in mind, *x*, is indeed *F*. Then attribute to the speaker beliefs about *x* that are too inadequate - ignorance - or too wrong - error - to enable her to supply the completion demanded by the explicit approach or to delimit the domain of quantification as demanded by the implicit approach. I applied this recipe to describe briefly some examples (pp. 301-302). One can afford to be brief because it is really very easy to develop the examples into plausible counterexamples to Russell.[14] And yet in each development, despite ignorance and error, the speaker successfully uses an incomplete definite to express a thought about a particular object in mind, *x*.

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[14] A defender of Russell may be tempted to go parasitic at this point, allowing the speaker to do the job with a description like 'that I am perceiving' which specifies the causal-perceptual relation that, in the view of the referentialist, does identify *x*. This temptation should be resisted because it is theoretically redundant (pp. 300, 302).

If this is right then pragmatic hypotheses like (i) are wrong. According to these hypotheses, a speaker who uses 'The *F*' in 'The *F* is *G*' referentially to refer to *x* would be saying that *p* where *p* is a certain general proposition. And the problem posed by incomplete definites is that *there is frequently no general proposition that the speaker might plausibly be thought to have said*. Everyone agrees that the speaker *means*, at least partly, the singular proposition that *x* is *G*. Given that, why might she be *saying* a general proposition? There are two possibilities: (a) She might mean that proposition *as well as* the singular one. (b) Even though she does not mean the general proposition, saying it might be a way of conveying the singular one just as in a metaphor saying something you don't mean is a way of conveying what you do. The discussion of incompletes shows straightforwardly that (a) is not the case. Where 'the *F*' is obviously incomplete, as it very often is, the speaker clearly does not believe or mean to say that there is an object that is uniquely *F* and is *G*; for example, that there is one and only one table in the world and it is covered with books. And the arguments from ignorance and error show that she is often not in a position to modify that general proposition, by completing 'the *F*' or delimiting its domain, into one that she might plausibly believe and mean. So, we are left with (b): the speaker does not mean the general proposition but nonetheless says it to convey the singular one. But how could saying it do that? *We need a plausible account of why the speaker would think that saying the general proposition will convey the singular one and why the hearer would take the saying of the general one to convey the singular one.* Where the general proposition *identifies x*, the subject of the singular proposition, we might be able to give an account. But the discussion of incompletes shows that she is often not in a position to say a general proposition that would identify *x*. Where she is not, I suggest, there is no reason for her to think that saying a general proposition will convey the singular one. I conclude that the generalization that a person using a definite referentially says a general proposition whilst implicating a singular one is false. We shall return to (b) when discussing Bach in the next section.[15]

This is my main criticism of pragmatic hypotheses like (i). But I think the hypotheses have two further problems.

First problem. Suppose such a hypothesis were right. Then it would have to have been the case that, historically, the quantificational convention for 'the', hence the attributive use of definites, *came before* their referential use. For that convention features in pragmatic explanations of the referential use. A person has to be already able to exploit that convention to say the general proposition, in order to convey the singular one. The problem is: Do we have any reason to believe that the attributive use did precede the referential use?

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[15] It is time to take account of a complication we set aside earlier (sec. 3, note 10). In giving his account of conversational implicature, Grice talks not only of a person "saying" that p but also of his "making as if to say" that p. He is led into this, as Bach points out (2001), by his infelicitous stipulation that saying that p entails meaning that p. Yet we often seem to say what we don't mean; think of metaphors, slips of the tongue, and so on. I agree with Bach that we should drop this stipulation and hence broaden 'saying' to cover what Grice intended to capture by 'making as if to say'. And that is what my argument does in taking (b) as a possibility.

I don't doubt that if the attributive use did come first then the quantificational convention could be the basis of a pragmatic explanation of the referential use. But this alone does not give significant support to the idea that it did come first because, if the referential use came first, it would surely not be hard to come up with an explanation of the attributive use from the referential convention for 'the'. I doubt that we have any firm evidence about which use came first. Perhaps both uses arose together![16] If we do lack firm evidence on this historical matter, then that is a problem for any pragmatic explanation of referential uses.

Second problem. Neale notes that a referential definite functions "like a name or like a demonstrative" (1990, pp. 85-86). In "Case" I claim that when definites are used referentially, a complex demonstrative would usually serve the communicative goal well enough (pp. 288-289). And often the speaker could have used a name, a simple demonstrative, or pronoun. So, when a definite is used referentially, there are nearly always other devices available that, according to the conventions governing them, can to do the referential job. (Bach disagrees, but in section 7 I shall argue that he is wrong.) If the referential use of descriptions was indeed not a semantic convention we would expect that use to be rare in situations where there is another device available that is conventionally used referentially. Yet that use is far from rare in such situations; indeed, referential uses of incomplete descriptions are ubiquitous. Given the usual availability of other devices for expressing singular thoughts, why would we, day in and day out, use descriptions for that referential purpose if it were not their conventional role to be so used? This is *another* argument for RD. (It is, in effect, "Argument II" in "Case", pp. 287-288.)

I turn now to consider Bach's defense of Russell. This consideration adds to the case for RD.

6. *Bach's pragmatic defense of Russell*

The defender of Russell has two important tasks. On the positive side, he needs to provide an argument that referential uses are akin to indubitably pragmatic phenomena. On the negative side, he needs to criticize the argument that the uses are semantic, pointing out, for example, how they differ from what is uncontroversially semantic. Bach attempts both. I shall now look critically at these attempts.

First, however, let me start on a happy note of agreement. Bach's conception of "singular thoughts" is as follow:

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[16] Stephen Neale informs me that his research suggests that 'the' came from 'that'. My student Boone Gorges informs me that his research suggests that 'the' and 'that' have a common origin in a word of uncertain function. Another student, Francesco Pupa has drawn my attention to the following: "Modern English 'the' is a continuation of the Indo-European pronominal stem *to. The function was not originally that of an article; we meet the word in historic times as a demonstrative pronoun." (Christophersen 1939, p. 84)

We can have singular thoughts about objects we are perceiving, have perceived, or have been informed of…The connection is causal-historical, but the connection involves a chain of representations originating with a perception of the object. Which object one is thinking of is determined relationally, not satisfactionally… there must be a representational connection, however remote and many linked, between thought and object. (pp. 191-192)

This is very much the same as my view (p. 290; 1981a, b). And it is an important piece of the background. Now on to the disagreements.

I claim that definites are regularly and standardly used referentially. Bach agrees (p. 201). He would also agree that my semantic explanation of this is a *possible* one: definites might have referential meanings. In a benign mood, he might even agree that this semantic explanation is beautifully simple. But he thinks that he has a pragmatic one that is better: "I take referential uses to be akin to *generalized* implicatures" (p. 227). The heart of this explanation is to be found in his Point 5:

The distinctive quantificational character of definite descriptions helps explain how and why they can readily be used to refer, *because it plays a key role in their referential use.* (p. 201; emphasis added)

This point also provides the basis for his main criticism of my explanation: "if definite descriptions are ambiguous, their ambiguity is most extraordinary: one of their senses (the quantificational) plays a role when the other (the referential) is operative" (p. 224).

I shall start with Bach's Point 5 and then respond to his criticism.

Note first that Bach's view that the quantificational character of definites helps explain how they can be readily used to refer requires that that character came before that ready use. For, it has to be already present to do the explaining.[17] This raises the first problem discussed in section 5. Do we have any reason to believe that the history meets this requirement?

Set that aside. Bach thinks that the quantificational meaning of definites does more than explain the development of referential uses, which is quite compatible with RD, it still "plays a key role in" that use. So, the idea is that the best explanation of what is now going on in a referential use involves the quantificational meaning. And this makes a referential use akin to a conversational implicature. I don't think that Bach's explanation is a good one.

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[17] Bach may be right in claiming that "to endorse the Russellian claim…does not commit one to the view that their attributive use came first and their referential use somehow developed later" (p. 226). But any pragmatic *explanation* of the referential use, such as his, does seem to have the commitment.

Here, briefly, is my rival explanation. There is a semantic convention of using 'the *F*' to refer to *x* which exploits both a causal-perceptual link between the speaker and *x* and a meaning of '*F*' (which might be ambiguous). So the truth underlying Bach's claim is that a descriptive meaning of '*F*' plays a role in what is said. But, of course, a descriptive meaning of '*F*' is not the same as a quantificational meaning of 'the *F*'. Let us fill this out a bit. In a referential use, a singular thought is conveyed. Bach agrees that a person's singular thought is about a certain object in virtue of a causal-perceptual link to that object. So such a link accompanies all referential uses. It is then plausible to think that it plays a semantic role, just as it does with a deictic pronoun or demonstrative (on which more in section 7). A speaker expressing a singular thought about a certain object participates in the referential convention and thus exploits the causal-perceptual link to that object; a hearer participates in the referential convention and thus takes account of clues to what has been thus exploited. (This is not to say that either has a *theory* of the link, even, to use a popular weasel word, a "tacit" one. The capacities to exploit such links and recognize their exploitation are linguistic *skills*, pieces of knowledge-how consisting mostly of subconscious states that function automatically and speedily; see section 3.) Bach rightly points out that "singular definite descriptions…imply uniqueness" (p. 210).[18] But that implication need not come from a quantificational meaning; it can also come from a referential meaning, as it does with deictic uses of pronouns, demonstratives, and, I am claiming, referential descriptions.

Return to Bach's explanation. He has this to say about the "key role" of the quantificational meaning in the referential use of a description:

> The speaker thinks of a certain object, takes that object to be the *F*, and uses 'the *F*' to refer to it. The [hearer], on hearing 'the *F*', thinks of a certain object that he takes to be the *F*, and takes that to be what the speaker is referring to. (p. 203; see also his later example on pp. 223-224)

> one cannot understand a referential use of a definite description without grasping its literal, quantificational meaning (p. 226)

What does this amount to? We should note first that there is a way of understanding these claims *about the hearer* that is uncontroversial and quite compatible with RD. A consequence of RD is that a definite is ambiguous. Now, the evidence about a hearer's processing of *any* ambiguity – think of a boring example like 'bank' – is that both meanings are entertained, mostly subconsciously, before one is eliminated with the help of contextual clues (Gernsbacher & Kaschak 2003). So RD would predict that, when faced with a referentially used definite, a hearer will typically entertain its quantificational meaning before eliminating that meaning with the help of contextual clues.[19] But this is clearly not the key

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[18] Bach also rightly points out that 'the' alone indicates *totality.* It indicates uniqueness only when combined with a *singular* nominal (p. 202).

[19] Bach has some helpful things to say about this (pp. 198-204 *passim*, p. 224; cf. Devitt 1981b, pp. 521-522).

role for the quantificational meaning that Bach has in mind. For one thing, if it were, then there would be nothing in the least "extraordinary" about the ambiguity that is a consequence of RD (*cf.* p. 224). For another, there is no plausible analogous way of understanding Bach's claim *about the speaker*.

Initially, we might suppose that Bach has in mind a much more striking role for the quantificational meaning. The speaker's route from the singular thought that x is G to the use of 'the F' to refer to x in 'the F is G' is *via* the general quantificational thought that, according to Russell, is expressed by 'the F is G'. So, the quantificational meaning is *what is said*. And the hearer's route to understanding the referential 'the F is G' is via thinking that quantificational meaning. The quantificational meaning can play this role because it *identifies* the referent of a referential use.

This might seem immediately plausible if the 'F' in uses of 'the F' to refer to x typically applied *uniquely* to x. But this plausibility should disappear when we note that 'F' in referential uses of 'the F' typically does not apply uniquely and hence could not identify anything. As we have been emphasizing, most uses of definites are of incomplete ones. If the quantificational meaning were playing a role in what is said by a referentially used 'the F' it would indeed imply uniqueness,[20] as Bach insists, but given incompleteness it could not *achieve* uniqueness: it does not apply to one and only one object. As Bach says, "the speaker does not intend the description by itself to provide the hearer with the full basis for identifying the referent" (p. 221). And Bach rejects the idea that the definite "contains some hidden modifier that would make it complete or some phantom variable of domain restriction" (p. 203); he rejects the explicit and implicit approaches which we have just seen fail anyway because of ignorance and error problems. So our initial interpretation of Bach is clearly not right.

How then, on Bach's view, *does* what the speaker and hearer do in a referential use of an incomplete definite involve its quantificational meaning (beyond the just-noted uncontroversial involvement in the hearer's understanding that this *is* a referential use)? RD agrees with Bach that the descriptive meaning of the nominal 'F' plays a role in a referential use. And that meaning is *part of* the quantificational meaning of 'the F'. But, of course, the quantificational meaning goes beyond the descriptive meaning, implying that there is a unique F. Bach's whole case against RD rests on his Point 5 claim that the quantificational meaning plays a role in a referential use but, so far as I can see, *he says absolutely nothing* about what that role is (beyond the role of the descriptive meaning of 'F'). He talks of one object being "salient or contextually relevant", of it being the one the speaker "intends the listener to think of" (p. 203). But the quantificational meaning of the incomplete 'the F' quite plainly does not *make* that object salient, relevant, or intended since it does not *identify* the object. And Bach has not shown us that the meaning does anything else to communicate the singular thought. The quantificational meaning indicates Fs but Bach

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[20] Although, as I have noted, it is not the only meaning that implies uniqueness: referential meanings do too.

needs to show that it is otherwise relevant. He has not done this and so his Point 5 claim is left unsupported. More generally, Bach has failed to provide what a pragmatic account of referential uses must provide: an explanation of *why* the speaker would think that saying the general proposition will convey the singular one and *why* the hearer would take the saying of a general one to convey the singular one.

Suppose that Bach were right and so what is said by a predication involving a referential use of 'the *F*' is a general proposition about whatever is uniquely *F*. Suppose further that it is obvious to speaker and hearer that 'the *F*' is incomplete and so it is not plausible that the speaker *means* that general proposition. Nor is it plausible that the speaker *means* some enriched general proposition that might be obtained by completing 'the *F*'. Since the speaker obviously means *something*, a hearer will then try to identify some other proposition that the speaker might mean. Bach rightly claims that the speaker "intends and can reasonably expect the hearer to take him to be talking about a certain *F* that is identifiable in the context of utterance" (1994, p. 126). But why would the speaker *think* that saying a general proposition about whatever is uniquely *F* would fulfill that expectation? Why would it lead the hearer to identify a singular proposition about that *F* that the speaker has in mind rather than a singular proposition about any other *F* or, indeed, some other general proposition? And why *would* it lead the hearer? What would be the route from the general proposition said to the singular one meant? Plausible answers would involve something like a Gricean derivation of the singular proposition. Bach does not provide such answers. I doubt that there are any.

So, what *does* "provide the hearer with full basis for identifying the referent"? Bach's talk of "salience", "relevance", "what the speaker intends", and so on, simply labels the problem without solving it: *In virtue of what* is a certain object salient and so on? A solution cries out to be heard. The object is salient and so on in virtue of a causal-perceptual link to the thought that the speaker is expressing. And *what provides the needed identification is the referential meaning of 'the F', a meaning established by the convention of exploiting causal-perceptual links between thoughts and objects*, just the same links that are exploited by similar conventions for demonstratives and deictic pronouns. *The referential use of a definite, like the use of a demonstrative or pronoun, makes the object of thought salient to the hearer because she participates in the appropriate referential convention*. With this answer we abandon Bach's Point 5 explanation and arrive at mine. And we do so by adverting to something that Bach himself thinks accompanies all referential uses: a causal-perceptual link to the object in mind. Bach clearly agrees that the speaker and hearer exploit this link to identify the referent. But whereas RD's view that 'the *F*' has a referential meaning enables us to explain *how* uttering 'the *F*' exploits that link, Bach's view that 'the *F*' has only a quantificational meaning leaves the exploitation unexplained and inexplicable.

Finally, I turn to Bach's main criticism of RD. That criticism is that "if definite descriptions are ambiguous, their ambiguity is most extraordinary: one of their senses (the quantificational) plays a role when the other (the referential) is operative" (p. 224).

The failure of Bach's pragmatic explanation enables us to deal with this criticism very swiftly. The quantificational sense is (mostly) *not* operative in referential uses except in the earlier-discussed uncontroversial way in which all meanings of an ambiguous expression are typically operative in a hearer's process of understanding. So there is nothing extraordinary about the ambiguity.

In sum, Bach has not provided an adequate pragmatic explanation of the standard regular use of descriptions to refer. The RD's simple semantic explanation is still the best.

7. Bach on the comparison with complex demonstratives

Building on the Argument from Convention, "Case" argues that definites are like deictic complex demonstratives in two ways. First, they have a very similar conventional function in the expression of a singular thought about an object. Since the convention for demonstratives is semantic, so too is the convention for definites. Second, when used according to these conventions, a definite and a demonstrative both depend for their reference on the causal-perceptual link of the thought to the object ("Argument III", pp. 288-290).[21]

Bach's discussion of his Point 3 shows that he has a somewhat different view, a view that he seems to think (pp. 224, 228) supports his pragmatic view of the referential uses of definites against the semantic view. According to his Point 3, "often the only way to refer to something is by using a definite description":

> Suppose you want to refer to some thing (or someone). Suppose it is not perceptually present, has not just come up in conversation, and is not otherwise salient. Suppose that it does not have a name or that you are unaware of its name or think your audience is unaware. Then you cannot use an indexical, a demonstrative (pronoun or phrase), or a proper name to refer to it…Your only recourse is to use a description. (p. 198)

Suppose that Bach were right about this. So we would often be in the position of having singular thoughts to convey with only one way to convey them: use a description referentially. So we would expect descriptions to be used referentially in those circumstances, as Bach points out. But we would surely expect something more. We would expect that, over time, this way of conveying singular thoughts in those circumstances would become the conventional way. We would have a clear need for descriptions to have a referential meaning and so we should expect them to develop that meaning. Bach would have kindly provided the basis for an argument – certainly not conclusive, but quite powerful nonetheless

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[21] In response to this argument, Bach wrongly claims that "Devitt simply assumes that [complex demonstratives] have referential meanings" (p. 228). I give a brief argument (p. 291). Bach also cites King 2001. A response to this book must be left to another time.

– *for* descriptions having a referential semantic convention! There would be no support here for his pragmatic explanation of referential uses.

Sadly, I must reject this argument. I don't think we are often in the situation Bach describes. In particular, I have a problem with his suggestion that you can use a description referentially to refer to *x* in situations where you could not use a "demonstrative phrase". So the idea is that you could use 'the *F*' even though you could not use 'that *F*'. I claim (pp. 288-289; see also 1981a,b) that, near enough, whenever you can use the one you can use the other. I don't doubt that there are a few situations where one expression is appropriate and the other not. Nor do I doubt that there are a few others where one will seem *more* appropriate than the other. Still, in the latter situations, the other could almost always be used without undue strain.

In disagreeing with me about this, Bach describes two situations of interest. The first is one where Bach thinks that a demonstrative is appropriate but a definite is not. There are many books on the table. Pointing at one, the speaker says, "I haven't read that book". Bach claims that the use of definite 'the book' would have been "pragmatically anomalous" (p. 228). But it is very easy to fill out the description of the situation so that it would not have been: we just have to think of situations where the demonstration accompanying the utterance is not necessary to make the book in question salient; for example, the speaker and hearer might be both looking at the book and the hearer might be about to pick it up. It would be perfectly appropriate for the speaker to say, "I haven't read the book" (with or without a demonstration), perhaps continuing "Still I have heard great things about it". There is nothing anomalous about this. The truth underlying Bach's claim is that the more a demonstration is called for to increase salience, the more natural it will seem to use a demonstrative rather than a description. I acknowledged this difference (p. 288).

The second situation is one where Bach thinks that the reverse is the case: a definite is appropriate but a demonstrative is not. His wife says to him, "The DVD player is broken". Bach claims that 'that DVD player' would not be usable in this situation (pp. 198-199).[22] On the contrary, if the DVD player was salient – because his wife was looking at it, for example, or because the difficulties of operating it had been the subject of conversation last night - his wife could have served her purposes *just as well* with the demonstrative. And even if it was not particularly salient, she could have done *nearly as well* with the demonstrative. This example suggests the following generalization: the less salient the object is - where salience is a matter of prominence in the current perceptual environment (perhaps because of previous actions) or prominence in current thought as a result of past experiences - the more the strain in using a demonstrative.

I do not deny Bach's claim (p. 228) that "there are clear pragmatic differences" between definites and complex demonstratives although, as we have just seen, he exaggerates those

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[22] This claim, and the DVD example, were not in what I understood to be the final version of this part of Bach's paper when, referring to that paper, I wrote: "Bach offers no examples to support his claim that there are situations where a description ('the *F*') could by used to refer but a complex demonstrative ('that *F*') could not". (p. 289n)

differences. However, in thinking that this claim counts against my Argument III, Bach misses the key point of the argument. The key point is not that the roles of two expressions are *the same* but that they are *very similar*: in most situations the two expressions are interchangeable without any strain at all. This is a good reason to think that in their referential uses descriptions, like demonstratives, have a referential meaning.

In sum, even if Bach's Point 3 were right and "often the only way to refer to something is by using a definite description" that would still not support a pragmatic view of referentially used descriptions, as Bach seems to think it would. Rather it would support a semantic view, RD. But Point 3 is not right. The main argument for referential definites comes not from their being often the only way to express singular thoughts but from their being frequently and standardly used to express singular thoughts. That is my Argument I, the Argument from Convention. Furthermore, RD explains a striking fact: definites are used to express singular thoughts despite there nearly always being other devices – a pronoun, demonstrative, or name – available for that purpose. That is my Argument II, briefly described in discussing the second problem in section 5. Finally, the similarity between deictic complex demonstratives and referentially used definites gives further support to the view that those definites participate in a semantic convention. That is my Argument III. Whether demonstratives differ from definites as much as Bach thinks they do, or as little as I think they do, there is no support for his pragmatic view.

Let us take stock. Before discussing Bach, section 2 summarized the Argument from Convention focused against the view that referential uses are *particularized* conversational implicatures, an argument presented in detail in "Case". Partly on the basis of ideas in that argument, the next three sections developed an argument against the Gricean view that referential uses are *generalized* conversational implicatures, indeed against all pragmatic explanations. First, Grice's characterization of conversational implicatures does not enable us to distinguish these from what is said. Our modified characterization, particularly the move from talking of the hearer's *competence* to derive the implicature to talking of her *actually* deriving it, may succeed with this distinction but, at first sight, seems no help in assessing the Gricean view that referential uses are generalized conversational implicatures. Still, second, one feature of the distinction counts against that Gricean view. Indeed, it counts against *any* similarly pragmatic hypothesis. The feature is that any pragmatic explanation of these uses will claim that the speaker conveys a singular proposition while saying a general one. A consideration of incomplete definites shows that this requirement can frequently not be met: there is no general proposition that the speaker might plausibly be thought to have said. This point was further developed in the discussion of Bach, which strengthened the case for RD. All in all, that case strikes me as fairly overwhelming.

I turn finally to Neale's latest view.

8. Neale's illusion

Neale accepts the Argument from Convention and no longer holds a thesis that was central to *Descriptions* (1990): that "the difference between *saying* and *meaning* lies at the heart of a characterization of referential usage". So one might have hoped that he would embrace RD and the view that definites are ambiguous. But he does not. He still thinks that "the Russellian analysis is basically correct for both attributive and referential uses of descriptions" (p. 106). Furthermore, he now thinks that debate over RD "seems to lack real substance" (p. 71); it "is the product of a powerful illusion" (p. 106).

At the center of Neale's new view is the idea of "Gödelian completions" used in spelling out Gödel's slingshot argument (Neale 2001). According to the basic Russellian view of definites, the only contribution that 'the *F*' makes to the truth conditions of the sentences containing it is as a unique applier. To deal with incomplete descriptions, we noted (sec. 5) that this basic view had to be enriched: the unique application may be by a supplemented 'the *F*' (explicit) or as a result of restricting the domain of 'the *F*' (implicit). Ignorance and error arguments showed that this will not, in general, work for referential uses (sec. 5). Neale's Gödelian completions avoid this difficulty (pp. 171-173). His proposal is that a referential use of 'the *F*' invites the Gödelian completion, 'the *x*: *x* is *F* and *x* = *a*', where '*a*' is either a simple demonstrative or a name referring to the object the speaker has in mind.[23] The Gödelian completion is to be treated in the standard Russellian way, hence the claim that the Russellian analysis is "basically correct". So, to that extent, the conservative "Unitarian" school was right. But the radical "Ambiguity" school was also right to a certain extent. For, the referential use is "a special case" of incompleteness, it is "highly regular, perhaps even conventional" (p. 173). He is even prepared to allow that it may be "part of the *meaning* or *semantics* of 'the' that on one use it invites a Gödelian completion". So, "in a sense, everyone was right and everyone was wrong" (p. 172), hence the claim that the debate is "the product of a powerful illusion".

I have three responses to Neale's new view of referential uses. (i) I shall emphasize how much the Gödelian proposal concedes to the RD side of the debate. (ii) I shall urge that what it doesn't concede, it should. (iii) I shall claim that it is Russellian only in a technical not substantive sense. So the debate is not an illusion: RD has won.

Four claims define RD. The first is:

> (1) The uncontroversially different referential and attributive uses of 'the *F*', one to convey a singular thought about a particular *F* object in mind, the other to convey a general thought about whatever is uniquely *F*, participate in different semantic conventions. The latter convention is Russellian but the former is not.

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[23] Notice that this proposal is not guilty of the parasitism that I criticize (pp. 300, 302). It does not involve describing the perceptual-causal link but rather including a demonstrative element in 'the F' that exploits that link.

Consider the sentence 'The *F* is *G*' where 'the *F*' is incomplete. Neale agrees that there are two distinct regular uses of definites with the result that this sentence yields propositions with two distinct truth conditions. On the regular referential use it yields a Gödelian completion, a proposition equivalent to

$$(Ex)(Fx.x=a.Gx),$$

where '*a*' is a referential device that the speaker has in mind to pick out the referent. On the regular attributive use it yields one equivalent to the very different

$$(Ex)(Fx.Hx.(y)(Fy{\rightarrow}y=x).Gx),$$

where '*H*' is a predicate the speaker has in mind to compete the definite or restrict its domain. Neale does not commit to the view that these two regularities are distinct semantic conventions but he is prepared to go along with that view. For the reasons presented in this paper, I think that he should go along.

If the regularities are semantic conventions then the only respect in which Neale might be at odds with (1) is in his claim that the referential use is Russellian. But is the Gödelian completion *really* Russellian? Note that it yields a proposition that is *equivalent* to a conjunction of singular propositions:

$$Fa.Ga.$$

So it is hard to see how the Gödelian completion yields something that is in any *interesting* sense a general proposition and hence Russellian. We might say that it is syntactically Russellian but not semantically so; it is only "pseudo-Russellian". Neale himself remarked earlier:

> A phrase of the form '[the *x: x = a*]' is technically a Russellian definite description; but the claim that referential uses of descriptions do not require distinctive non-Russellian interpretations would indeed be hollow if the Russellian position could be maintained only by employing the identity relation to concoct descriptions of this form (e.g. '[the *x: x =* that]'). (1990, p. 115 n. 53)

The hollowness is demonstrated by the fact that when a "concocted" description of this form is joined to the predicate '*G*' it yields a proposition that is equivalent to 'That is *G*'. And the hollowness of Neale's present claim that, because of the appeal to Gödelian completions, the Russellian position is "basically correct" is similarly demonstrated by the equivalence of '*(Ex)(Fx.x=a.Gx)*' to '*Fa.Ga*'.

The next claim that defines RD is:

(2) The referential convention for definites exploits a perceptual-causal link to the object in mind to achieve uniqueness.

Neale does not explicitly embrace (2) but doing so would fit well with his views. For, he has earlier accepted, a little tentatively, the view that an "object-dependent" thought – what I am calling a "singular" thought - is about its object partly in virtue of its perceptual-causal link to the object (1990, p. 18).

(3) The referential convention for definites is very similar to that for a complex demonstrative.

Neale says of complex demonstratives that they are "Gödelian *by nature*. An act of reference is signaled *as a matter of linguistic convention*" (p. 174). 'That *F* is *G*' yields a proposition equivalent to

$$(Ex)(Fx.x=\text{that})$$

The proposition yielded by the referential 'The *F* is *G*' differs from this only in having '*a*' in place of the simple demonstrative 'that', where '*a*' stands in for *either* 'that' *or* a name. But I can see no motivation for this difference and the discussion in section 7 counts against there being the difference. If one takes the Gödelian route for referential definites (but see 4 below), then we should take the referential 'The *F* is *G*' as expressing the *same* proposition as 'That *F* is *G*' (the differences between them being merely pragmatic). As I noted in "Case", we should treat the referential 'the *F*' as "implicitly containing something like the simple demonstrative 'that'" (p. 292).

This modification of Neale's proposal further undermines the idea that the Gödelian route for referential definites is genuinely Russellian. That idea gets support from the claim, reflected in the very name "Gödelian completion", that referential definites are typically *in*complete. So the suggestion is that they are just like the indisputably Russellian attributive definites in typically needing to be completed, albeit in a different way. Now there is no doubt that referential definites are typically incomplete in the usual sense of not uniquely describing an object. But it is important to see that if my modification is correct they are not incomplete in another sense: they are not *semantically* incomplete and so do not need to *be* completed to convey the intended message. Referential definites are *already* semantically complete, as complete as complex demonstratives. Like the demonstratives they already have underlying them the causal-perceptual link that determines reference. If we take the Gödelian route, referential definites are as "Gödelian *by nature*" as complex demonstratives.

(4) The '*F*' in the referential 'the *F*' is semantically significant.

"Case" (p. 292) mentions three ways of making '*F*' semantically significant but remains neutral on the choice between them.[24] The ways are: (i) '*F*' is partly determinate of the

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[24] Although in earlier works (1981a,b) I have favored (i). Bach notes my neutrality in "Case" but then strangely continues: "In this regard it is not clear that his discussion precludes an account on which 'the *F*' (on its alleged referential reading) means the same as 'the particular *F* I am talking about/thinking of'; a description that is uniquely satisfied by

reference of 'the *F*'; (ii) 'The *F* is *G*' is equivalent to 'That is *F* and *G*'; (iii) the modified Gödelian route just discussed. Neale's unmodified Gödelian route clearly gives semantic significance to '*F*' and so is quite compatible with (4).

In sum, Neale's Gödelian proposal, as it stands, is close to RD. It would become even closer if he accepted, what should be congenial, that the regular referential uses constitute a semantic convention and that this convention exploits a causal-perceptual link. It would then *become* RD if he accepted, as I have argued he should, the modification that takes referential definites to be like complex demonstratives. Finally, this proposal is not in any interesting sense, Russellian: it is only pseudo-Russellian. So the debate over referential uses of definites is not an illusion: RD has won. Neale's contrary view reminds one of Senator Aiken's witty advice to President Johnson when the Vietnam War was going very badly for America: "Just declare victory and go home."[25]

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the unique object the speaker has in mind" (p. 225). It surely *is* clear that my discussion precludes this account: I say I am neutral between the three possibilities (i) to (iii) and this is not one of them! Furthermore, I *argue against* just such an account – an account analogous to causal descriptivism for proper names (which is criticized by Devitt and Sterelny 1999, p. 61) – on the grounds that it is theoretically redundant (pp. 300, 302). You could roughly characterize my view as that the referential meaning of 'the *F*' is provided by the (causal-perceptual) link to the object thought about together with the contribution of '*F*'. A proposal like Bach's is an example of a description theory that is parasitic on this sort of causal theory. According to the causal theory, most of the meaning of 'the *F*' is determined by *a link* to the object. According to a parasitic description theory, most of the meaning is determined by *an associated description of that link* to the object. In my paper I remarked that, so far as I knew, no such parasitic theory had been proposed for referential descriptions. I went on to say that, "given the history of theories of names and demonstratives, it seems likely that one soon will be" (p. 300). It didn't take long!

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